SCIENCE

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CONTENTS

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THE SCOPE AND RELATIONS OF TAXONOMIC BOTANY[1]

IN his famous work, "Philosophia Botanica," Linnæus, in accord with his fondness for system in all things, classifies the authors that have dealt with botany and allied subjects. He first divides botanical writers into two groups, true botanists, and botanophils or lovers of botany. The botanists are again divided and subdivided with much detail into numerous groups. The botanophils consist of four groups, the anatomists, the gardeners, the writers upon medicine, and lastly a miscellaneous group including those who write upon plants from the standpoint of economics, panegyrics, theology or poetry. It is clear from this classification that among those who concerned themselves with plants the systematic botanists held the dominating position. They were the real botanists, the others were only botanophils. Among the latter were the few anatomists and physiologists such as Malpighi, Grew and Hales. It is true that Linnæus has, as a subdivision under the true botanists, the heading physiologists, but he defines these as those who reveal the laws of vegetable growth and the mystery of sex in plants. This disposition of the physiologists was no doubt influenced by Linnæus's own interest in sexuality in plants. In this connection it should be noted that the great classifier places Hales, the physiologist, among the botanophils and not among the botanists. For a century more the botanical

[1] Address of the retiring president of the Botanical Society of America, Columbus, December 29, 1915.

field was dominated by the taxonomist. But at this time taxonomy meant for the most part only the study of the species of the flowering plants. During the nineteenth century other branches of botanical science asserted themselves and began to compete with taxonomy for supremacy. Toward the end of the century taxonomy not only lost its dominance, but in this country at least was relegated to an inferior place. At present the pendulum indicating the trend of botanical thought has swung far away from the position occupied during the days of Linnæus, Hooker, Torrey and Gray. Taxonomic botany in the conventional sense is almost taboo. There is a feeling abroad among botanists that systematic botany is old-fashioned and that to be a taxonomist is to be behind the times. At most of our institutions of learning taxonomic botany as such is not taught at all or is relegated to a minor position. At the meetings of our botanical societies the percentage of papers dealing with taxonomy is disproportionately low. In a recent number of the *Plant World* it was stated that out of the 45 doctorates in botany conferred by American universities in 1915, two were taxonomic. The same disproportion prevails in most of our journals and periodicals devoted to the whole field of botanical science. It is difficult also to obtain properly trained young men for positions in taxonomic botany.

Let us examine the causes that underlie these conditions, first, the dominating position of systematic botany during the eighteenth and early part of the nineteenth centuries, and second, the gradual loss of prestige since the middle of the last century and the inferior position in botanical thought now occupied by systematic botany as represented by the classification of species.

The botany of the seventeenth century consisted almost entirely of the enumeration of the known species of plants. Up to this time, the chief interest in plants, leaving out of consideration of course the crop plants, had been their use in medicine. From the practical standpoint it became necessary to record the known species and to describe their uses. These records are preserved in those massive tomes generally classed as herbals.

As the number of known species increased, attempts at classification were made, crude at first but reaching an advanced stage in the eminently practical sexual system introduced by Linnæus. There was early a reaching out for a natural system of classification. Even Linnæus, the creator of the sexual system, tells us in his famous text-book, cited above, that a natural classification is diligently to be sought, that plants show affinities on all sides like a territory in a geographical map.[2] He then proposes 63 groups of flowering plants and four others to include the ferns, mosses, algæ and fungi. The first work to use a natural system on a large scale was Decandolle's "Systema,"[3] the first volume of which was issued in 1818. The natural system soon displaced the Linnæan system.

The years from 1790 to 1850 were an era of botanical exploration. Thousands of plants from all quarters of the globe poured into the centers of botanical research. There was much work for every botanist who was capable of distinguishing and describing species. Every collection received

[2] *Phil. Bot.*, 27, 1751. "Methodi naturalis fragmenta studiose inquirenda sunt. Primum et ultimum hoc in botanicis desideratum est. Natura non facit saltus. Plantæ omnes utrinque affinitatem monstrant, uti territorium in mappa geographica. Fragmenta, quæ ego proposui, hæc sunt."

[3] Decandolle, A. P., "Regni vegetabilis systema naturale," 1, 1818; 2, 1821.

from exploring expeditions yielded scores or even hundreds of new species. Is it to be wondered at that taxonomy held a dominating position in botanical research and that most of the great botanists of the period were taxonomists? At the beginning of this period modern chemistry and physics were in their infancy and the compound microscope had not yet been perfected. It is easy to see why it was that well along into the nineteenth century botany was considered to be chiefly the description and classification of species. Furthermore, botanists were working upon the hypothesis of created species, not upon that of the evolution of species.

Let us turn back for a moment to note the growth of experimental science. Of science as we understand it there was little in the middle ages. Instead was authority. Arguments were settled, not by direct test, but by consulting the statements of the Fathers. Tradition hampered the development of all branches of human research, especially of the sciences. The statements of Aristotle were for ages considered infallible. That the root of the mandrake cried with pain when torn from the earth and that the fruit of the goose tree developed into bird or fish according as it fell upon the land or in the water, was accepted without question. Here and there a courageous soul by questioning tradition created much disturbance and brought upon himself the revilings or at least the reproaches of the powerful. Slowly at first, more rapidly during the latter part of the eighteenth century, grew the tendency to test theories by experiment, to verify facts. Great minds were attracted to the field of scientific research. There was a certainty about the relation between cause and effect that was vastly satisfying to the intellectually alert. The discovery of oxygen by Priestly in 1774 gave an impetus to chemistry. Upon the foundation of facts accumulated up to about the time that Davy decomposed potash in 1808, Dalton was able to present a theory of the construction of matter, the atomic theory, which to a remarkable degree has answered up to the present time the requirements of a working hypothesis even though modified by recent discoveries. Faraday, the great experimenter, somewhat later laid the foundation for the extraordinary development in the domain of electro-magnetics. The microscope was greatly improved, opening up a wide field of research concerning the internal structure of plants. During the nineteenth century there was much interest in plant anatomy, in the development of the cryptogams, and finally in plant physiology. A growing proportion of botanists devoted their attention to these or allied branches. The theory of the special creation of species was superseded by that of the evolution of species. As the experimental method was widened in its application to the various fields of botanical research, the science of botany became increasingly attractive to intellectual workers. But in this increased interest and activity descriptive taxonomy has not retained its share. At the beginning of the nineteenth century nine tenths of the prominent botanists were engaged in the discrimination of species. At the beginning of the twentieth century probably not one tenth are thus engaged. This reversal of proportion is due in part to the widening field of botany. Still I think that there is at present in descriptive taxonomy an evident lack of interest that is not entirely explained by this widening of the field of botanical research.

What are the reasons for this lack of interest in what is conventionally known as systematic botany? Some botanists, especially of the younger generation, have be-

littled descriptive taxonomy because this seemed the popular thing to do, having received the impression that taxonomy was old-fashioned. Some, also of the younger generation, have come little in contact with the subject during their period of training, hence consider it a very special line which is rather a side issue compared with such subjects as morphology and physiology. Some have felt that a career in systematic botany offered little in the way of reputation or of financial return, and so have entered more promising fields.

Another reason for the somewhat suspicious attitude assumed by some botanists toward taxonomy is the prominent part assigned during the last twenty-five years to nomenclature. The creditable desire to place nomenclature upon a sound basis has resulted in many changes in familiar names. Such changes have been embarrassing to morphologists and physiologists who look with disfavor on changes in terminology except in their own branches. Furthermore there have been auxiliaries who have taken advantage of the unsettled condition of nomenclature to substitute the study of names for the study of plants.

These reasons, however, are inconseqential. They would deter no one imbued with the scientific spirit. Is not the fact that there has been no satisfactory way of applying the experimental method to the discrimination of species the real reason why descriptive taxonomy has been avoided by so many botanists? In chemistry and physics the relation between cause and effect can be tested and results can be foretold. In recent years the same method, answering questions by direct test, has been applied in many branches of botany. But in taxonomy one has no definite test by which results can be proved. One may put in years of hard work and seem to get nowhere. You will remember how Charles Darwin struggled with the classification of barnacles and Asa Gray with asters. I thoroughly sympathize with them, as will every botanist who has attempted a serious study of the classification of a difficult group of plants. We work over them for months, patiently noting differences and resemblances, assembling and segregating, seeming to have a scheme nicely worked out, only to have it upset by a new batch of specimens, going through all the stages of hopefulness, satisfaction, doubt, hopelessness, and finally tearing our hair and exclaiming "Confound the things! What's the matter with them, anyway?" This kind of work does not appeal to the average scientist. He prefers to wrench facts from nature by frontal attack, by applying the experimental method. He wants to do something under controlled conditions and see the result, having the assurance that under the same conditions he will always have the same result. To him this puttering over the differences of species is the veriest waste of time—that is, of his time. His attitude towards the classification of species is much like the attitude of descriptive taxonomists towards the classification of horticultural varieties. If the horticulturist classifies the hundreds of varieties of the apple by such characters as color, form, size, markings and time of ripening, the groups will merge into one another. The resulting classification can be used only by those who know the varieties; but if they know the varieties they do not need a classification by which to identify them. This is somewhat exaggerated, but it fairly well represents the way the classifier of species looks at the work of the classifier of horticultural varieties. And I think it represents the way the physiologist looks at the work of the taxonomist. This is not said in disparagement of the work of classifying horticultural varieties. It is given

only as an example of how such work suffers by being far removed from the field of experimental research.

The taxonomist arrives at results not by the application of the experimental method, but by the repetition of observations. To be sure the geneticists are applying the experimental method with considerable success, but their results can have no immediate bearing on the subject under discussion. Ascertaining facts by the method of repeated observations lacks the precision and definiteness of the experimental method. The examination of hundreds of herbarium specimens, plant mummies, is not so fascinating nor so satisfying as it is to set up a piece of apparatus and see something happen. I believe this is the chief reason why so many of our keenest minds have hesitated to join the ranks of the descriptive taxonomists, the results appearing to them indefinite in proportion to the time and energy spent in obtaining them.

Now let me review with you the scope of taxonomy in the broad sense, the science of classification. To me the two great questions that botanists seek to answer are, *how do plants live?* and *how are plants related?* Most botanical investigations can be used as an aid in answering one or the other of these questions. From this standpoint the two fundamental divisions of botany are physiology and taxonomy. Many facts in physiology may be established by experiment. Most facts in taxonomy are established by repeated observation. Various subsidiary branches of botany may aid one or the other of these fundamental divisions according as the facts obtained are used in answering the main question. Morphological studies gather certain facts which in themselves are interesting, but which reach their highest usefulness only when structural morphology yields to physiology and comparative morphology yields to taxonomy.

Taxonomy in the general sense is the science of classification. But the taxonomy with which we are concerned is that which attempts to answer the question, how are plants related. The very question implies that plants are related. Our taxonomy assumes the evolutionary hypothesis that all the organisms of the present day have developed or evolved from other somewhat different organisms of the past. The great truth which taxonomy is seeking to express is the genetic relation of organisms. If the genetic history of all organisms were known the classification of these organisms would be merely an arrangement of facts. But the genetic history of organisms is not known, or known only for an infinitesimal number for an infinitesimally short period of time. Our classification of plants is, then, an expression of judgment as to what are the probable genetic relations of these organisms. At the best this classification can represent only a cross section of the lines of phylogenetic development. It may be compared to a formula in calculus with a large number of variables. The value of the formula may be found for any given moment by substituting the values that the individual variables have at that same moment.

Some workers in this broad field of systematic botany are studying the relations of the more comprehensive subdivisions of plants such as the families, orders and groups higher than these. In such investigations they seek for resemblances, as it is these upon which, rather than upon differences, the interpretations of relationship must be based. Some botanists, on the other hand, are concerned chiefly with the determination of the relations of the ultimate systematic groups of organisms, the species and their subdivisions. Here the observer

is seeking differences, for the resemblances are plain to be seen.

Intermediate between the species and the family lie groups, such as the genus and the tribe, in which one must look for both differences and resemblances and strike a balance between them. The botanist who studies species, and hence looks for differences, compares the more superficial characters of plants, those that are most easily modified in the development of new forms. The botanist who studies the relation of orders and more comprehensive groups, is concerned chiefly with those characters which have resisted modification in the course of development. I have referred to the work of the former as descriptive taxonomy. The work of the latter is often included in the designation comparative morphology. It is to be regretted that there has been some lack of understanding between these two groups and a consequent lack of sympathy. A title such as "The Morphology of *Cycas* and *Welwitschia* and its Bearing on the Origin of the Gymnosperms" would attract one group as being an important paper in comparative morphology, while a title such as "Five Hundred New Species of *Rubus*" would be laid aside with the remark, "another species-maker broken loose." The one looks upon the other as a species-maker who knows little of the real problems of botany. The other looks upon the one as a section-cutter who knows plants only through the compound microscope. Both may be doing really good taxonomic work. This is determined, however, not by the fact that one is cutting sections and the other is describing species, but by the fact that each is using scientific methods and is dominated by the scientific spirit.

The ideal of the taxonomist is a scheme which shall represent the genetic relations of organisms. Each of us who are taxonomists is hoping to contribute his mite toward this harmonious whole. The lines of descent are real though unseen; they exist, but their position and direction can not be proved. As the astronomer studies the constitution and evolution of the universe, as the chemist studies the constitution and evolution of matter, so the taxonomist studies the constitution and evolution of organisms. One of us may be describing new species of *Rubus,* and showing their relation to previously known species. Another may be revising a genus of mosses and adjusting the relations of the species in the light of recently acquired knowledge. Another may be studying the comparative anatomy of seaweeds and with the observed facts attempting to solve the problem of relationship. Another may be studying the development of the spores of smuts and tracing, as it were, the prehistoric development of the group. Thus are anatomy, morphology, ontogeny, paleontology, yielding facts to taxonomy. Thus are we all uniting in the great effort to answer the fundamental question, how are plants related.

Considering the trend of botanical thought during the present decade, I need not ask the members of this society if comparative morphology is an interesting field of research. My predecessor and my successor are shining examples of those who have advanced the limits of our knowledge in this branch. But the domain of descriptive taxonomy, the elaboration of genera and species, is this an inviting field for the young botanist who is seeking an opportunity to take part in solving the problem of relationships?

For reasons already given this branch has been unpopular in recent years. Though descriptive taxonomy will never attract workers to the proportionate extent that it did before the rise of the experimental sciences, yet it will be found to satisfy the

cravings that are characteristic of the students of science. The mental satisfaction of the scientist who loves his work comes not alone from achievement, but from the actual doing.

There is the search for truth, the discovery of facts, the arrangement of the facts to represent relationships, the peering behind nature as she is to determine how she came to be what she is, the blending of all into a harmonious whole, the feeling that we are solving one of the fundamental problems of the universe. Our imagination bids us soar aloft in the realms of speculation, but our progress in these delightful journeys is ever limited by the chains of facts binding us to earth. We are victims of the inexorable law of compensation. We must pay the price for successful flights in this realm of speculation where we dream our dreams and build our theories. This price is the drudgery of slowly accumulating facts. We must work, work, work. We must examine thousands of specimens, both living and preserved, in herbarium, garden and field, just as the comparative morphologist must examine thousands of sections, staining, cutting, mounting. We must measure and weigh evidence, persistently, patiently, accurately. It is thus that we lengthen the chains binding us to earth and thereby soar higher and higher. Occasionally one of us is so exhilarated with the joy of soaring that he severs the chains of facts and rises unrestricted, but, alas, soon disappears from view. Some of us are so busy with our facts that we never have time for soaring. Only a few have that happy combination of industry and imagination that allows them to rise to great heights and yet remain within our view. These few have that rare ability to select related facts, to distinguish the essential from the non-essential, to separate the significant from the insignificant.

The laboratory of the descriptive taxonomist is three fold, the field, the herbarium and the garden. The facts concerning the plants that he studies can most satisfactorily be observed from living specimens in their native habitat, that is in the field. Apart from the practical impossibility of observing in the field all the plants that the worker may wish to study, there is the difficulty of comparing those that grow in different localities. This difficulty is in part overcome by bringing together in a herbarium preserved specimens. The disadvantage of studying herbarium specimens is that in the larger individuals only a part can be represented and that many characteristics of the living plant can not be shown. By far the most satisfactory method of studying plants from widely separated localities is to bring them together in a botanic garden where they may be preserved alive. It is almost hopeless to attempt the study from herbarium specimens alone of such groups as cactuses, palms, agaves, and bamboos. I can not let this occasion pass without emphasizing to you the importance to taxonomy of having a national botanic garden. Under the supervision of the federal government such a garden is likely to receive more ample support than one depending upon state, municipal, or private aid, and because of its national character is likely to extend its influence over a wider area.

There is another phase of descriptive taxonomy—an eminently practical one—which merits attention, namely, certain relations between this branch and all other branches of botany, relations which involve the use of the botanical names of plants. Taxonomy is the classification of organisms, but definite progress requires the use of names for the organisms classified. Names are older than systems of classifica-

tion. First there were vernacular names in the different languages. Then vernacular names in Latin acquired prominence because Latin early became the language of the books. Later we have the development of the idea of the genus with the corresponding application of the generic name, the kinds or species being distinguished by descriptive phrases. And finally Linnæus introduced the use of the trivial or, as it is now called, the specific name. Thus each species is designated by a binomial. You will readily see that plant names become, as it were, units of precision by which all branches of botany are standardized. From this standpoint taxonomy is fundamental because it furnishes the standard units of comparison and coordination, these units being not merely the names but the ideas which these names represent.

Botanists not always have been sufficiently impressed with the necessity of basing results upon carefully prepared standards. If a chemist wishes to determine acidity by titration he first prepares standard solutions of acid and alkali; or if he wishes to determine the atomic weight of zinc he first prepares pure zinc or zinc salt as a basis from which to work. If a surveyor wishes to cover a country by triangulation he first measures with extreme accuracy his base line. Results can not be compared unless they are based upon an accurate common standard. Suppose a chemist wishes to determine the solubility in water of sodium carbonate at different temperatures and to compare his results with those obtained by a chemist in another country. Suppose he is well trained, accurate in his methods, has balances weighing to a small fraction of a milligram, determines the solubility of his glass vessels, and the purity of his distilled water, and is able to calculate results within small limits of probable error. Then finally suppose that he bases all his careful work on a bag of washing soda obtained at a corner grocery. What would you think of him? Suppose an American botanist wishes to repeat the investigations made by an English botanist upon the anatomy of the stem of the day lily. Suppose that he is well trained, accurate in his methods, has the finest of microtomes, has at his command the last word on staining and methods of imbedding, supports his record with magnificent photomicrographs, and is a master of all the technique required. Suppose he is not acquainted with the day lily, but nevertheless trusts his untrained gardener to bring for his investigation a plant which the latter thinks probably is a day lily. What would you think of him? I have attempted to illustrate my point by exaggerated examples. I wish, however, to emphasize the statement that all comparative investigations upon plants depend for their usefulness upon accuracy in the identification of the species compared. Even competent and experienced botanists have sometimes neglected to establish at the beginning of an investigation this firm basis for work. The less experienced man is sometimes inclined to assume, especially if he has had limited training in taxonomy, that plants in gardens, greenhouses and herbaria belong to the species indicated by the labels they bear. Such faith is beautiful to behold, but, alas and alack! the worker is often a victim of misplaced confidence. The investigator should first establish the identity of the plant which he studies. If he has not had sufficient training in systematic botany to enable him to do this himself he should refer his specimen to a competent taxonomist and preferably to a specialist in the group to which the plant belongs. In comparing results based upon definite species of plants there arises

another uncertainty. Supposing that the second of two investigators whose results are to be compared, has based his work on correctly identified material, can it be assumed that the first investigator has taken equal pains to identify his material? Unfortunately this assumption may be without sufficient foundation. One must consider the probability of error. The paper recording results may show internal evidence of a satisfactory nature. The author may state the origin of his material or note the care taken in its identification, or that it was submitted to a specialist. Any such internal evidence increases confidence in the results. If the plants used by both investigators of this hypothetical pair have been accurately identified we may apply the mathematical axiom, things equal to the same thing are equal to each other. However, the probability of error is very greatly reduced if direct comparison can be made. This can be done only if each investigator has preserved the plants he has studied. This leads me to make this plea to botanists. Let every worker preserve the specimen he has studied if his results are in any way connected with the identity of the species. I think that anatomists, cytologists, morphologists, and others that study the internal structure of plants, are in the habit of preserving in alcohol or other liquid, the portion of the plant with which they have worked. Specimens of this kind should always be preserved in order that observations may be confirmed, but fragments, such as these are likely to be, are not usually sufficient for taxonomic identification. For the latter purpose a specimen should be prepared and placed in a public herbarium, accompanied by a label bearing the data necessary to connect the specimen with the investigation that it supports. If such supporting evidence is at hand any controversy as to the identity of the plants studied by different workers can be settled by consulting these herbarium specimens. The physiologist may find it to his advantage to follow the same procedure. His work is often with plant life in general rather than with particular species. But whenever his investigations concern definite species he should preserve herbarium specimens. The ecologists are fond of giving lists of plants growing under certain conditions and comparing these plants with those growing under similar conditions elsewhere. In the early days of this younger branch of botanical science little attention was paid to the identity of the species, and still less to preserving representative specimens. The subject was lightly waved aside with the assertion that they were not concerned with the identity of the individual species, only with the aspect of the vegetation. The modern school of ecologists, I am pleased to say, takes a more serious view of the rôle played by definite species. If it is worth while publishing a list of species at all, it is worth while supporting the record with permanent evidence. The geneticists, a young and active brood of investigators, will find it to their advantage also to adopt the method outlined above. The living specimens are the best of evidence while they exist, but at best they are evanescent. Herbarium specimens, if properly prepared and properly cared for afterwards, are permanent. If his plants have been passed upon by a general taxonomist or better by a group specialist the non-taxonomist may be deluded with the idea that his record is complete, that the identity of his species is beyond question and is fixed for ever and ever. Such an assumption depends upon the infallibility of taxonomists. I can assure you, however, that taxonomists are very fallible. Specialists may not agree among themselves on the identity of a

given plant, and the same specialist may not agree with himself on the identification of the same plant made at different times. This is not said to discredit the specialist. But specialists in taxonomy like specialists in other lines are not, even though specialists, masters of all the knowledge of the group of plants they study. Their opinions may change as their knowledge increases. Then let me repeat, the only safe way to support records when definite species are concerned is to preserve specimens and place them in a public herbarium.

The names of plants are the common language connecting all sciences and arts having any relation to botany. For a large part of the botanical public, consisting of agriculturists, horticulturists and many botanists, especially those who are not taxonomists, the usefulness of taxonomic work lies in the ease and certainty with which botanical names can be applied. To them names are convenient symbols by which plants are known. A change in the application of botanical names is as confusing as the change of a person's name. Consequently they look with concern and disfavor upon the seemingly kaleidoscopic changes undergone by the names of common plants. After the publication of the "Rochester Code" there was a rush to bring plant names in accord with this code. Some of this work was serious or at least sincere. Some was such as to leave the impression that the authors had in mind chiefly the publication of new combinations. The flood of new names appearing in lists, local floras and isolated notes, the work based upon a study of books rather than of plants, produced an unfavorable effect upon the standing of systematic botany. Those unfamiliar with the real scope and meaning of taxonomy hastily concluded that this branch of botany was for triflers, was not worthy of serious study, and was to be avoided.

But we should not be confused by superficialities. For example, an enthusiastic youth, wishing to climb the ladder of fame, makes a voluminous list of plants growing in swamp, in prairie and in forest, and inflicts upon the public, "The Ecology of Podunk." His brother, with an equally laudable purpose, delves in some musty volumes, consults the Index Kewensis, and emerges with a list of brand-new combinations, after each of which appears his own name as the authority. Let us not judge the scope of ecology by the incomplete efforts of the one, nor the scope of descriptive taxonomy by the misdirected efforts of the other.

Nomenclature is an essential detail in all taxonomic work. One should not hesitate to change a name if there is a necessity for a change. It has been said that a name is an expression of a taxonomic idea. Nothing should stand in the way of the most precise expression of correct taxonomic ideas. While it is desirable to conserve familiar names it is a poor policy to avoid change merely to conserve names. The objection, then, is not to the study of nomenclature as a detail in connection with monographic work, but to its study apart from the study of the organisms to which the nomenclature applies. There is even objection to work in which a superficial consideration of organisms is merely a series of pegs upon which to hang an elaborate study of nomenclature. Changes in names should be evidently a result of serious study of the group concerned.

The non-taxonomic public is constantly pleading with the taxonomists "to get together," to agree on a system of nomenclature which shall result in the stability of plant names. The taxonomists, I may say, are sometimes impelled to voice the same

sentiments in so far as concerns changes of names in groups of plants of which they have no special knowledge. Several attempts have been made to legislate upon the subject of nomenclature. It has been impossible thus far to frame a set of rules to which all botanists can agree. There are the rules of botanical nomenclature formulated at the International Botanical Congress held at Vienna in 1905. These rules are often referred to as the Vienna Code. To many competent botanists in both Europe and America these rules are so unsatisfactory that they will not subscribe to them. In this country many botanists have agreed upon a code, usually known as the American Code, which from the practical standpoint is more certain in its application. These two codes provide that our nomenclature shall begin with the year 1753, the date of the publication of the first edition of the "Species Plantarum" by Linnæus. There are still other botanists who would throw aside all limitations to the rule of priority and use the earliest names to be found in literature. Recently some one proposed a new name for the genus *Zizania* because *Zizania* of Linnæus, the swamp grass called wild rice, is not the same as *Zizanion* of the New Testament, which is the name of the weed the enemy came and sowed and which in our version is called "tares." It is not my purpose here to discuss these systems of nomenclature. I am only calling attention to the lack of unanimity on the subject among taxonomists. But suppose all taxonomists should agree upon a single system of nomenclature. Would this do away with the changes of names? By no means. In the first place it would take years to adapt the hundreds of thousands of names of plants to any code that might be adopted. But aside from these changes coincident with the search through countless books, pamphlets and ephemeral sheets, some very rare, some probably unknown to the present generation of botanists, aside from these changes due to the imperfections of our records, there are other changes resulting from the increase in our knowledge of plants. Stability in nomenclature is unattainable, just as stability or permanence in any branch of learning is unattainable so long as our knowledge concerning that branch is increasing. Codes of nomenclature enable botanists to make changes according to definite rules, they do not eliminate change. We shall have stability of nomenclature only when we have stability of taxonomic ideas, which latter will come only with infinite knowledge.

This society includes a large percentage of the botanists of this country, physiologists, morphologists, taxonomists, paleontologists, ecologists, cytologists, anatomists, geneticists, pathologists, but all botanists, and all contributing to the upbuilding of the science of botany. The society might be compared to a living organism, in which each botanist is performing a definite work contributing to the success of the society, even as each organ, or each cell, performs a definite function necessary or helpful to the life of the organism. By far the greater part of our work consists in accumulating details. As a successful army can not consist solely of generals, so a successful botanical society can not consist solely of philosophers. As the great general is one with an extended knowledge of the duties of his subordinates, so the true philosophical botanist must be intimately acquainted with much of the detail of the worker, the drudgery of small things.

When first we enter the realm of botanical research we long in the impatience of youth to make some great discovery, to reach at a single bound the heights to which others slowly toil. As we grow older we

realize that we must "build the ladder by which we rise." Each of us finds that he is but one of a vast army of patient plodders, seekers after truth. We become more and more willing to do that which is close at hand, to seize small opportunities as they pass, rather than waste time looking for the great opportunities of our dreams. Darwin was one of our great speculative philosophers, but his philosophy was founded upon an amazing array of facts, and his experience as an observer of details, especially that gained in his classic taxonomic investigation of the barnacles, contributed in no small degree to the soundness of his philosophical judgment.

Though the realm of botany, as a whole, is too great for any one individual to comprehend all its branches, and each must confine himself to one or two branches, the sympathy of each may and should extend to every branch. Finally, the ideal of taxonomy is the utilization of the results obtained by all the branches of botany; it is the expression of the sum of the knowledge to which all contribute; it is the philosophy of botany in that it correlates the parts into a harmonious and ever growing whole.

A. S. Hitchcock
U. S. Department of Agriculture

THE CENTIGRADE THERMOMETER

The Hon. Albert Johnson, member of Congress from the third district of the state of Washington, under date of January 12, addressed to members of the American Association for the Advancement of Science the letter which follows:

A reprint of my speech "Abolish the Fahrenheit Thermometer," dealing with Bill H. R. 528, introduced by me on December 6, 1915, is sent herewith to all members of the American Association for the Advancement of Science.

The speech is followed by extracts from letters, and I profit by this opportunity to express my sincere thanks to the writers of those letters for the valuable aid which they have rendered. I request that this acknowledgment be accepted in lieu of a personal reply, which I am reluctantly compelled to forego, owing to lack of time and clerical help.

The labor and expense involved in this undertaking will at best be considerable. Already the expense for printing exceeds $150. While every step should be taken with due deliberation, any unnecessary delay would involve a regrettable increase of labor and expense. If no action is taken at this session of congress, much of the work will have to be done over again at some other session. No man that has any regard for his reputation will care to say that the irrational, inconvenient Fahrenheit scale ought to be maintained; the only question is, how soon it should be abolished. An amendment lengthening the transition period to 8 or 10 or 15 years may be worth considering, but we should ill deserve our reputation as a progressive nation if we delayed to *set a date* for the abolition of a daily felt nuisance. As pointed out by several correspondents, it ought to have been done long ago. The change will necessarily be attended with considerable inconvenience, but this will not be lessened but increased by delay. We have already earned enough ridicule by clinging so long to the worst thermometric scale.

Every man in a responsible position now has a chance to gain credit by doing his best to facilitate the change. If any should feel tempted to advocate delay, they ought to consider that they would thereby gain not credit but discredit, because the change is sure to be made in the near future.

The Pan-American Scientific Congress has twice recommended "the establishment of the Pan-American Meteorological Service." Evidently the first requisite for that purpose is the abandonment of the Fahrenheit scale.

It appears that the government departments have authority, under existing law, to discontinue the use of the Fahrenheit scale. In publications designed for the scientific public, many bureaus do use the centigrade exclusively. However, as regards publications intended for the general public, it is evident that the departments would expose themselves to severe criticism if they made the change without an express mandate from congress. Congress evidently will not act except in response to an unmistakable demand on the part of the scientific public.

All progressive scientists, therefore, should unite to rid American science of this "iron shirt of